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TESTING GAME-THEORETIC MODELS OF FREE RIDING: NEW EVIDENCE ON PROBABILITY BIAS AND LEARNING

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No. 549 April 1990

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I. Introduction

The free rider problem is endemic to political processes. Political activists bear organizational costs for the benefit of a large (free-riding) membership. Nonvoters can avoid bearing informational costs and transactions costs with little risk of affecting the outcome of an election. Congressmen can avoid taking an unpopular stand on an issue (say, raising congressional salaries), if enough other congressmen are willing to. In international politics, lesser powers can benefit from the policing activities of the mighty. At not quite so grand a level, professors who avoid onerous committee and teaching assignments benefit from these activities because some colleagues agree to serve.

All of these problems have in common the following features. There is a group, all of whose members stand to benefit from the generous contributions of some subset of the group. They are all examples of *public good problems.* They have been studied extensively both theoretically and experimentally in most fields of social science. In this paper, be present both theory and experiments about a particularly simple class of public good problems: voluntary contribution threshold games (Van de Kragt et al,1983, Rapoport,1985, Lipnowski and Maital, 1983, Palfrey and Rosenthal, 1984, 1988, 1989). In these games, a group benefit is produced if at least w of N players "contribute" their endowments. An individual obtains the highest possible payoff (when $w \neq N$) as a successful free rider--the good is produced without the individual having contributed.

Models of this kind of public goods problem that could eventually apply to natural settings should, we argue, incorporate some element of private information. Each individual will generally be incompletely informed about certain characteristics of other individuals. Here we model this uncertainty as pertaining to endowments. How much an individual values his own endowment, relative to the public good, is private information to the individual. Endowments values are randomly and independently assigned according to a probability distribution that is common knowledge. In the formal model of decision-making, subjects are assumed to be risk

neutral.The predicted outcomes under the assumption that players maximize expected earnings are provided by game theory.

Game theory makes "rational expectations" predictions about behavior.In our case, behavior reduces to a single binary decision -- contribute the endowment or keep it.In order to make this decision, each player assesses the likely behavior of the other players. In our setting, the relevant assessment is of the likelihood that each of the other players will contribute. Players then optimize given these expectations about the behavior of the other players.[1] An equilibrium of the game imposes the following rational expectations hypothesis: each player's optimization given their probabilistic expectations generates the probability distribution over behavior that everyone had been anticipating. This is called a Bayesian Nash Equilibrium of the game of incomplete information. Corresponding to any equilibrium, there will be a dual prediction about (1) the probability distribution over outcomes, and (2) the decision rules adopted by the players.

This paper presents findings from a large number of laboratory experiments and compares the behavior in these experiments with the predictions of the theory. We report three major findings.

First, the equilibrium predictions from game theory are very accurate, at least at an aggregate level. On a qualitative level, as we vary the treatments, the observed outcomes always move in the direction the theory would predict. On a quantitative level, the efficiency of actual public good provision almost exactly mirrors the numerical predictions of the theory. Furthermore, the level of voluntary contribution is usually very close to the predicted level of contribution. Moreover, once subjects gain familiarity with the rules of the game, we do not observe the kind of patently irrational behavior (violation of dominated strategies) often observed in other public goods experiments.

Second, to the extent that we observe variations from the theoretical predictions, there is a relatively simple model with is very successful in accounting for the error. Specifically, for some treatments we observe overcontribution, while for others we observe undercontribution. For the most part, these deviations are consistent with subjects behaving

approximately optimally, but assuming that other players are more "civic-minded" than they are. In other words individuals underestimate the probability that others free ride. We compare this explanation of the variations from the theory to a variety of other possibilities, based on incomplete experimental control over utility functions. These alternative explanations fare poorly in comparison to the probability bias model.

Third, we find little evidence that individuals update their inaccurate beliefs about the other subjects' inclinations to free ride. While we are unable to cleanly reject the hypothesis that individuals are learning about other individuals' behavior, we find the paucity of support for such a learning hypothesis surprising. Nevertheless, in spite of the apparent rigidity of beliefs, the data still are remarkably supportive of the equilibrium predictions from noncooperative game theory.

While we thus want to emphasize that we found mostly small deviations from the theory, the deviations do require further explanations. We chose to pursue two alternatives within the basic framework of our decision-making model.

One possibility is that the observations may have been measured badly, which in our case means that some variables we thought we were controlling perfectly in the laboratory setting were in fact not perfectly controlled.The most obvious possibility would be that the players were not risk neutral with respect to the dollar payoffs, but had a non linear utility function over the outcomes, that might even depend upon the payoffs to the other subjects.[2]

A second possibility is that the rational expectations hypothesis might be wrong. This would not necessarily mean that players did not maximize expected utility, but that their subjective assessments of the other players' behavior was inaccurate or even systematically biased. We propose one particular hypothesis about a systematic bias in players' expectations about the behavior of the other players:

H: Individuals overestimate the probability that others contribute.

Equivalently, individuals underestimate the probability that others do not contribute. In looser terms, individuals underestimate the probability that others will be tempted to either free ride or to avoid the potential loss of their endowment. Here the hypothesis has been stated verbally. A precise specification of the hypothesis that will allow us to investigate its validity with our experimental data is provided later.

H leads directly to predictions about how behavior will systematically deviate from the predictions of (risk-neutral) game theory. For example, if only 1 of N individuals need contribute to produce the public benefit, overestimation will lead to a reduced level of contribution. The reason is that,in any 1 of N game, raising the probability others contribute raises the expected utility of not contributing but leaves the expected utility of contributing unchanged. The reasoning is reversed in the N of N game. There overestimation increases the expected utility of contributing but leaves the expected utility of not-contributing unchanged. More generally, in some games, overestimation is predicted to increase individual contribution; inother games, the prediction is reversed.

We specifically chose experimental parameter values--in terms of w, N, and the distribution of endowments relative to the value of the public benefit--that lead to a prediction of increased contribution and those that lead to a prediction of decreased contribution. In section 3, we show that the predictions developed from overestimation of probabilities cannot be replicated by non-linear utility models or by models of cooperative or altruistic behavior.

We were in fact led to develop the hypothesis of probability overestimation rather than non-linear utility on the basis of a set of experiments conducted at the California Institute of Technology in 1987-88 and Carnegie-Mellon University in early 1989. While game theory was reasonably successful in accounting for the outcomes of voluntary contribution games (without communication), there were some systematic departures from the predicted equilibrium rates of contribution. Depending on the experimental parameters, we found that sometimes there was significantly higher contribution rates than predicted and sometimes there was significantly less than predicted. After developing H, we designed

several "critical" experiments with different parameter values than the original experiments. We also replicated the original parameter values. In some new experiments, overcontribution, relative to game theory, was predicted; in others, too little. The results of these new experiments, conducted at the California Institute of Technology in the summer of 1989,also strongly support H.

Finally, most adaptive expectations models or Bayesian learning models would predict that if an experiment is conducted repeatedly in a stationary environment with random groupings, then subjects will update their expectations about other players' behavior in light of information from early play in the experiment. In fact there is a great deal of substantial evidence in favor of this elsewhere in the experimental literature (e.g. McKelvey and Palfrey [1989], Forsythe, Palfrey, and Plott [1982], Camerer and Weigelt [1988]). This has implications for the data, as well. For example, in contrast to the non-linear utility explanations for deviations from our predictions, the probability bias hypothesis, together with adaptive behavior, predicts that behavior will be closer to the Bayesian equilibrium predictions as the number of replications increases. We find some evidence supporting this, but it is very weak. Overall, there appears to be a great deal of persistence to the biased beliefs.

In Section II of the paper, we develop the game-theoretic model. In Section III we outline the model of overestimation of probabilities and some alternatives to the model of risk neutral selfish behavior. In Section IV, we present the experimental design and the results of the experiments. Concluding remarks appear in Section V.

II. The Equilibrium Model of Voluntary Contribution

A group consists of N persons. A group project requires at least w units of input. Each group member is endowed with one indivisible unit of input, which may be either "consumed" by the individual or "contributed" to the group project. The voluntary contributions game consists of a single simultaneous move in which each individual's choice set is the pair {contribute, not contribute}.

The project succeeds if and only if at least w units are contributed. The value of the project to any individual is normalized to equal 1. The private value of the endowed unit of input to an individual is denoted c_i. Each person knows his or her own c_i but only knows that the other players' c's are independent random draws from some common probability distribution with CDF $F(\cdot)$. Assume F is continuous and strictly increasing on $[0,\bar{c}]$, $\bar{c}>0$, with $F(0) = 0$ and $F(\bar{c}) = 1$.

The payoff for player i with endowment (cost) c is given by:

1+c	if	i does not contribute and at least w others contribute
c	if	i does not contribute and fewer than w others contribute
1	if	i contributes and at least w-1 others contribute
0	if	i contributes and fewer than w-1 others contribute[3]

No side payments are permitted.

Individual rationality makes $c \leq 1$ a necessary condition for contribution.[4] Thus, <u>if all costs were public information</u> (complete information) and fewer than w players had $c \leq 1$, the only equilibrium is for everyone to not contribute. Indeed, total non-contribution is always an equilibrium unless w=1.

If $k \geq w$ players had $c < 1$ in a game of complete information, there would be $\binom{k}{w}$ pure strategy equilibria where exactly individuals contribute. One of these, where the w lowest cost individuals contribute, would be the "efficient" equilibrium. (If side payments were permitted, it would always

be efficient for the w lowest cost individuals to contribute if $w\bar{c}<N$.) There are also be a multitude of equilibria where some or all of the players used mixed strategies (Palfrey and Rosenthal [1984]).

Because communication among the players is ruled out in our environment, there is no direct way in which players can coordinate or correlate their strategies or arrive at an outcome similar to the complete information efficient outcome.[5] Moreover, since the asymmetries between the players--that is, their different endowments--remain private information, asymmetries cannot be used, even tacitly, as a coordinating mechanism.

It is natural then to assume that all individuals will use an identical rule when they make their contribution decisions independently and simultaneously. As shown in Palfrey and Rosenthal [1988], a symmetric Bayesian equilibrium to this game always has a particularly simple form. For any beliefs that player i has about the other players' decisions to contribute, there is a unique best response strategy which is a cutpoint rule. Therefore, a symmetric equilibrium is simply characterized by a threshold cost level, call it c^*, such that contribution is optimal if $c_i < c^*$ and noncontribution is optimal if $c_i > c^*$. While there may be more than one equilibrium value of c^*, the mild regularity conditions imposed on $F(\cdot)$ guarantee existence of at least one such value. The set of all such equilibrium cutpoints is the set of all solutions (in c^*) to the following equation:

$$c^* = \binom{N-1}{w-1}\left(F(c^*)^{w-1}\left[1-F(c^*)\right]^{N-w}\right) \equiv \text{Prob}(k=w-1) \qquad (1)$$

where k is the number of contributors other than i.

The interpretation of equation (1) is that a person with a private value of c faces an opportunity cost of contributing equal to c. In order for c^* to be an equilibrium, it must be that everyone with a cost below c^* has opportunity costs sufficiently low so as to be better off contributing, given others are using the c^* decision rule and everyone with private costs greater than c^* have too high an opportunity cost. Therefore, individuals with a cost of exactly c^* must be indifferent between contributing and not contributing. Since the value of the group benefit was normalized to one,

indifference implies that the cost of contribution must equal the probability of being pivotal to the success of the group project. This probability is Prob(k=w-1). The possibility of multiple solutions to (1) poses a potential problem in evaluating the data from the experiments. However, for the experiments that we conduct, it turns out that there is a natural concept of stability of equilibrium that nearly always generates a unique equilibrium prediction with our range of parameters. We say that a Bayesian equilibrium is Expectationally Stable if the following tatonnement process converges to the equilibrium c*.

Let c_0 be some initial cutpoint in the open interval $(0,\bar{c})$. Suppose everyone started out using c_0 as their cutpoint rule. Then, on average, players will observe a frequency of contribution $q_0 = F(c_0)$. Next suppose that this results in each of the players having expectations q_0 about the likelihood that a randomly selected opponent will contribute. One could imagine this, for example as the outcome of a learning process after many repetitions with opponents using c_0. Then the best response under these expectations (q_0) is to use a cutpoint c_1 satisfying:

$$c_1 = G(c_0) = \binom{N-1}{w-1}\left(F(c_0)^{w-1}\left[1-F(c_0)\right]^{N-w}\right)$$

We say that c* is an Expectationally Stable Equilibrium (ESE) if there exists an open set $C^E(c^*) \subseteq [0,\bar{c}]$ such that, for all $c_0 \in C^E(c^*)$, $(c_0-G(c_0))(c_0-c^*) > 0$. We define c* as being a Globally Expectationally Stable Equilibrium (GESE) if it is an ESE relative to the open set $(0,\bar{c})$. Thus, a Bayesian Nash equilibrium to our game is GESE if the adjustment process moves in the direction of the equilibrium from any initial cutpoint that is above 0 or below the maximum possible cost, $\bar{c}$.

In the experiments we report on here F is always uniformly distributed between 0 and $\bar{c}$ so there is always at least one ESE, and with one exception, there is always a unique GESE. The exception occurs when w=N and $\bar{c} \leq 1$. In that case both $c^* = 0$ and $c^* = \bar{c}$ are ESE and there is also an unstable equilibrium between 0 and $\bar{c}$. For $c_0 < c^\circ$, $c^* = 0$ is the limit point of the adjustment process $G(c_t)$ while $c^* = \bar{c}$ is the limit point if $c_0 > c^\circ$. When w=N and $\bar{c} > 1$, $c^* = 0$ is a unique GESE. In all other cases

with $w > 1$, there are exactly two Bayesian equilibria, one with $c = 0$ and the other with $c^* > 0$. It is easily shown that $c^* = 0$ is unstable but the other equilibrium is a GESE. If $w = 1$ then there is a unique globally stable Bayesian equilibrium.[6]

There are also some asymmetric equilibria which may occur for some parameters, $\bar{c}$, w and N. An example of such an equilibrium would be one in which a particular subset (say player numbers 1 through w) <u>always contribute regardless of their cost</u> (i.e. $c^* = \bar{c}$ for these players) and the other members of the group never contribute ($c^* = \underline{c}$). This is possible as long as $\bar{c} < 1$. We do not consider these (or other) asymmetries.

Therefore, for nearly all of our experimental parameters we will have a unique stable symmetric Bayesian equilibrium, c^*, that provides a strong prediction about individual behavior:

P1. Given $c^*(N,w,\bar{c})$, predict i contributes if and only if $c_i < c^*$.

III. Explaining deviations from the theory

While P1 is roughly supported, we will see that many individual decisions are not in accord with the rule. The model might nonetheless be correct in the aggregate. This suggests the following hypothesis:

P2. The aggregate probability of contribution is $q^* = F(c^*)$.

A natural estimator of q^* is $\hat{q}$, the observed frequency of contribution in an experiment. Standard tests can then be used to see if this observed frequency differs significantly from q^*.

We will in fact find significant deviations, even in the aggregate, from the theoretical probabilities. In this section we discuss two different approaches to "rationalizing" these deviations, one based on relaxing the rational expectations restriction of the theory and one based on modifying the utility hypotheses of our equilibrium predictions.

A. The biased probabilities hypothesis

In the introduction, we proposed the hypothesis that individuals consistently underestimate the probability that others freeride, but otherwise behave roughly in accordance with expected payoff maximization. What does this hypothesis, H, predict about these deviations? There are several alternative ways to operationalize H to apply it to our data.One interpretation is simply that H implies that individuals believe that others act according to some $c^{**} > c^*$. That is expectations about the likelihood others will contribute exceed the *equilibrium* likelihood others will contribute. Using these beliefs, they then use a cutpoint c^+ which represents the optimal decision rule given beliefs c^{**}.

To study how $c^{**} > c^*$ will affect the (observed) decision rule c^+, it suffices (for small amounts of bias) to differentiate (1):

$$\frac{dc}{dq} \gtreqless 0 \text{ if } q \lesseqgtr \frac{w-1}{N-1} \qquad (2)$$

Let q^+ = the actual probability of contribution. (Note that $\hat{q}$ is a consistent estimator of q^+.) The relationship (2), evaluated at q^* immediately leads to the following predictions under H:

P3a. If w=1, $q^+ < q^*$.[7]

P3b. If w=N and $0 < q^* < 1$, $q^+ > q^*$.

P3c. If $1 < w < N$ and $0 < q^*$, $q^+ > q^*$ if $q^* < (w-1)/(N-1)$ and $q^+ < q^*$ if $q^* > (w-1)/(N-1)$.[8]

Our experimental design consists of some 1 of 3 experiments where we always expect undercontribution relative to the game-theoretic predictions, some 3 of 3 experiments where we always expect overcontribution, and some 2 of 3 and 2 of 4 where the prediction depends on the value of $\bar{c}$. We varied $\bar{c}$ to produce the appropriate contrasts.

For our design, except in the unanimity game w=3, N=3, if $c^* > 0$, c^* is unique.[9] In unanimity games, there is no $q^* > 0$ when $\bar{c} > 1$, and a unique solution $c^*=1$ when $\bar{c}=1$. When $\bar{c} < 1$, we have one solution with $c^* = \bar{c}^2$ and a second solution $c^* = \bar{c}$.

Combining H with the auxiliary hypothesis that the bias is always with respect to the unique expectationally stable equilibrium thus leads to strong restrictions on observations. When multiple stable equilibria exists, we will assume that the bias is with respect to $c^* = 0$.

A second way to operationalize H is that players expectations about the likelihood others will contribute exceeds the *empirical* likelihood rather than exceeding the equilibrium likelihood. In other words the bias is with respect to actual contribution rates, not necessarily equilibrium contribution rates. Again, the measurement problem is that we only have data on contribution decisions, not on beliefs. However, for any observed rates of contribution, q^+, we can estimate a cutpoint decision rule, c^+ and infer what players beliefs would have to have been if they had been optimizing relative to those beliefs. Denoting these inferred beliefs by q^e, we then state

$$\text{P4:} \quad q^e \geq q^+$$

As we will discuss in the next section, this operationalization, while more appealing that P3 in some ways, has some drawbacks.

B. Alternative Models of Non-Linear Utility and Cooperation

The purpose of this section is to demonstrate that P3, the prediction developed on the basis of the probability bias hypothesis H, cannot be generated from plausible alternative models. We first deal with non-linear utility and next with cooperation.

Non-Linear Utility

Since our experimental subjects were paid in dollars, it is important to rule out utility being non-linear in money as an alternative explanation for our results. Without loss of generality, we assume that each player's utility for the outcomes is given by the function u with:

$$u(0) = 0;\ u(c) = c + \alpha(c);\ u(1) = 1;\ u(1+c) = 1+c+ \delta(c) \quad (3)$$

The case of risk neutrality is $\alpha = \delta = 0$. We continue to assume that all players have the same utility function[10] and that the functions α and δ are each either always weakly positive or weakly negative.[11] Again, our analysis focuses on local changes in the equilibrium. This directs our attention to $0 < c < 1$.

With (3), equation (1) generalizes to:

$$c^* = \text{Prob}(k=w-1) - \alpha \cdot \text{Prob}(k<w) - \delta \cdot \text{Prob}(k \geq w) \quad (4)$$

Let us begin by considering the common form of utility in economics, global risk aversion. This requires $\alpha > 0$ and $\delta < 0$. But in unanimity games, the last term in (4) must be 0. That is, the outcome with payoff 1+c is impossible in unanimity games. Therefore, $\alpha > 0$ implies that the utility of non-contribution is raised relative to the utility of contribution, in turn implying less contribution, contradicting P3b.

Having dealt with risk aversion, we turn to models where, in spirit with prospect theory in psychology, the utility function is partly risk-averse and partly risk-acceptant. These possibilities are covered by either:
(a) $\alpha > 0$ and $\delta \geq 0$, with at least one inequality strict, or
(b) $\alpha \leq 0$ and $\delta \leq 0$, with at least one inequality strict.

In case (a), the utility of non-contribution is raised relative to the utility of contribution. The utility function is risk-averse for lotteries between the two contribution outcomes (0 and 1) but risk-acceptant for lotteries between the non-contribution outcomes. Case (a) can be interpreted as the utility function for individuals who are loss averse with respect to the natural anchor formed by their endowment. They are risk averse for lotteries over the alternatives near the anchor point where there is a possibility of being worse off but are risk acceptant for lotteries that guarantee they will be no worse off than the endowment. Since the utility of non-contribution always increases, however, case (a) predicts that contribution always falls relative to risk neutrality. Therefore, case (a) is inconsistent with P3b and P3c.

In Case (b), the situation of case (a) is reversed. Now the utility of contributing is always increased and the individual is risk-acceptant for lotteries over the contribution outcomes. One can think of the individual as ascribing a non-monetary bonus or prize effect to the successful completion of the project. Alternatively, case (b) captures altruism effects, including the model of Palfrey and Rosenthal (1988). But since the utility of contribution always increases relative to risk neutrality, case (b) is inconsistent with P3a and P3c.

The remaining possibility is $\alpha < 0$ and $\delta > 0$. If $\delta \geq -\alpha c/(1-c)$, the utility function is globally weakly risk acceptant. We rule out this situation as a priori unacceptable. On the other hand, if $\delta < -\alpha c/(1-c)$, consider 1 of N games. In these games, the outcome 0 is impossible. The inequality condition then implies that the utility of contribution has been raised relative to that of not-contributing. Therefore, contribution should increase, contradicting P3a.

Cooperation

Another alternative view of behavior is provided by various models of cooperative behavior. An upper bound to what could be achieved by cooperation is the outcome that could be achieved by the players if they had full information about costs. This would have the w players with the lowest costs contribute if the sum of these lowest costs is less than N. In other words, the good is provided unless its total cost exceeds the total benefit. Another model of full information cooperation is for the w players with the lowest costs to provide the good as long as none of these costs exceeded 1.0, the value of the public good. Given that costs are private information, these highly coordinated behaviors cannot be achieved. Moreover, they lead to predicted contribution rates that are just very much higher than anything we observe in the data.

A more plausible model of cooperation is for players to use (perhaps as a tacit collusion against the experimenter) a cutpoint c^m that maximizes expected group payoff. This always implies a contribution rate $q^m > q^*$, where q^* is an ESE. As we will see in the Section V, q^m also substantially exceeds observed contribution rates.

In summary, we have established that models of non-linear utility (and models like altruism that can be interpreted in terms of non-linear utility) cannot be used to generate predictions equivalent to those of P3. To see if H is supported as a behavioral hypothesis, we turn to a presentation of our experimental design and results.

IV. Description of the Experiments

A detailed specification of each experiment appears as Table 1. The experiments at Carnegie-Mellon University used 72 undergraduates. The 1987 and 1988 experiments at Caltech used subjects who were undergraduates at Caltech or Pasadena Community college. The experiments in the summer of 1989 used high school students participating in a summer program at Caltech. The subjects were mainly male. None of the participants had prior experience in the tasks required in these experiments. Each experimental session used 9 or 12 subjects. A given experimental session consisted of one or three experiments, an experiment constituting a different set of parameters. In the CMU setting, there were six sessions involving three matched pairs. For the paired sessions, everything was identical except for the order of experiments. No important order effects were observed. Instructions are included as an appendix.[12] These varied slightly between CMU and Caltech and within Caltech sessions. Experiments run in 1987 and 1988 used a computer program written at Caltech. Experiments run in 1989 used a computer program written at Carnegie-Mellon, with minor modifications made at Caltech. None of the factors mentioned to this point appeared to have a notable influence on results.

All experiments were designed to have 20, 25, or 30 rounds. The number of rounds was made known to the subjects before the experiment started. Three experiments were inadvertently curtailed shortly before the planned end because of computer crashes.

At the beginning of each experiment, subjects were told w, N, and the value, in "francs", of the public benefit. They were also told how many cents they would receive, at the conclusion of the session. These values were held constant throughout an experiment.

In each round, subjects were each given a single indivisible "token" (endowment). Token values in franc increments between 1 and either 90 or 204 were independently drawn with replacement from identical uniform distributions and randomly assigned to subjects.[13] Each subject was told the value of his or her token but not told the values of the tokens of other subjects. Subjects were then asked to enter their decisions (spend

or not spend the token). If at least w of the N subjects spent, each subject received a number of francs equal to the value of the public benefit if he was a "contributor". If he or she was not a contributor, the payoff was this number of francs plus the token value. In table 1, $\bar{c}$ shows the rescaling of the distribution of token values such that the benefit is rescaled to 1.[14] Each round subjects were assigned to a new group in a rotation sequence which minimized the number of times any two subjects were paired together in the same group. The reason for doing this was to limit reputation and supergame effects which can occur with repeated play. Such effects appear to be very limited. In the ensuing analysis, we treat each decision as an independent observation.

V. Results

A. Testing the Bayesian-Nash predictions

1. Efficiency predictions

We begin discussion of the results by noting that the Bayesian Nash Equilibrium is remarkably successful as a predictor of aggregate outcomes of the experiments. For each group in each round of each experiment, we computed the actual earnings of the group. Using the actual token values drawn by the subjects, we also computed the predicted earnings for the group, assuming that play conformed to the theoretical cutpoints, c^*. We then computed average earnings per subject, normalized so that the public good had value 1.0 in each experiment. We then compared predicted earnings to actual earnings for each experiment. These are aggregate comparisons where individual variations from round-to-round and group-to-group are averaged out. (We maintain disaggregation across replicate experiments.)

The results offer remarkably strong support for the theory. For data from all rounds, the regression equation is:

$$\text{Actual earnings} = -0.046 + 1.040 * \text{Predicted earnings}, \quad R^2 = 0.95, \quad N=33$$

The estimated constant does not differ significantly from zero and the estimated intercept does not differ significantly from 1.0. The actual data points are plotted in Figure 1.

Since there is substantial variation in endowments (via $\bar{c}$) across experiments (endowments and actual earnings have an R^2 of 0.40), we checked that the success of the model was not largely driven by the correlation between earnings and endowments. Therefore, we also examined the increase in per subject earnings over per subject endowments. The results, plotted in Figure 2, continue to be striking:

$$\text{Actual Increase} = 0.012 + 0.932 * \text{Predicted Increase}, \quad R^2 = 0.93, \quad N=33$$

Again, at conventional levels, the intercept did not differ significantly from zero and the slope did not differ significantly from unity.

These impressive results occur even though the data, from other viewpoints, show systematic deviations from theoretical predictions. We discuss these deviations next with a series of tables where the rows show the value of $\bar{c}$ and the columns designate the four different w, N combinations used in the design. In each cell of the table, we present results for each experiment that matches the cell. The order of experiments within cells always matches the order given in Table 1. Solid lines in Table 1 separate cells in the later tables.

2. Individual Behavior

Table 2 shows that risk-neutral game-theory does not succeed at the individual level corresponding to P1. There are many errors of classification using the theoretical cutpoints. (For w<N, the non-zero cutpoint was used where it existed. For w=N=3, the zero cutpoint was used. More on this later.)

On the other hand, the prediction is qualitatively correct. Contribution rates are decreasing in cost, as shown by probit analysis that is not reported here. For all experiments, the standard χ^2 test for the 2x2 table contribute/not contribute vs. endowment above or equal to cutpoint/endowment below cutpoint, is significant at $p < 10^{-3}$. In the 19 experiments with a non-zero cutpoint where subjects had endowments above or at the benefit level, after excluding such subjects, the test is significant at $p < 10^{-4}$ except for p=.0882 in the CIT 7/26/89 2 of 4, =.0436 in the CIT 7/31/89 2 of 4, and = .0002 in the CIT 8/8/89 2 of 3. Thus, the prediction is qualitatively correct even if we exclude subjects who have weakly dominant strategies of not-contributing.

When a subject has a dominant strategy, the subject almost always acts in accordance with the model, as shown in Table 3. The only exception occurs in early rounds of a 2 of 3 experiment and a 3 of 3 experiment, both involving the high school students. The classification errors of the model

are thus almost entirely from the behavior of subjects without dominant strategies.[15]

3. Aggregate behavior

The game-theoretic model fares somewhat better at the aggregate level of predicting the frequency of contribution, as shown in Table 4. It should be noted that an important comparative static prediction of the game-theoretic model is strongly supported by the data. For w, N fixed, contribution is strongly decreasing in $\bar{c}$. As contribution is cheaper on average for low $\bar{c}$, contribution occurs more frequently as $\bar{c}$ falls. On the other hand, for $\bar{c}$ fixed, contribution is not increasing in w/N, which increases from left-to-right, in the table. A naive view of volunteering might claim that contribution should increase as w/N increases since a larger fraction of the group is needed to produce the public benefit. This naive view is not supported by the data.

If we examine each cell in the table, we find results that are sometimes in the "ballpark" of the theory, but we also find many statistically significant deviations, leading to rejection of P2. However, these deviations are mostly consistent with H. Consider first experiments with $w < N$. Of 25 experiments, all but six of them have observed contribution rates that are consistent with P3. Only one of the six inconsistent observations is statistically significant at the .05 level. In contrast, 15 of the 19 consistent observations are statistically significant. Experiments with w=N=3 can not be evaluated statistically because $q^* = 0$.(Relative to the 0 equilibrium, any positive contribution is consistent with H. On the other hand, there are also equilibria with 100% contribution rates for two of the cells. The observed contribution rates are obviously below those given theoretically and so, relative to the 100% contribution equilibrium are inconsistent with H.)

Results similar to those presented in Table 4 are obtained when we basis the analysis only on those subjects with dominant strategies. The results appear in Table 5. Two alternatives to H, non-linear utility that leads to "altruism" and cooperative behavior always predict contribution in excess

of the Bayesian Nash levels. In Table 6, we display the contribution probabilities q^m that would result if players followed the cooperative cutpoints c^m. (To provide readers with a sense of how parametric variation affects the probabilities, the probabilities are also shown for cells where no experiments were run.) The information in Tables 4 and 6 enables us to make comparisons across the 33 experiments. When $w = N = 3$ and $\bar{c} = 2.25$, both the cooperative and self-interested models predict zero contribution. (In this extreme case, the expected cost of providing the good exceeds the benefits.) In the other 31 cases, the observed frequencies are always closer to the Bayesian predictions than to the cooperative predictions except for the two observations where $w = N = 3$ and $\bar{c} = 3/4$. (For $w = N = 3$, we always made 0 the Bayesian prediction.) On the whole, the data represent strong evidence for self-interested behavior in comparison to full cooperation.

B. An alternative test of the probability bias hypothesis.

To this point, we have only examined the hypothesis about probability bias with reference to local properties the predicted equilibrium probability that a randomly selected individual will contribute. That is we began by considering a unique stable equilibrium cutpoint, then looked at the implied contribution frequencies if everyone adopts that strategy, then calculated the derivative of the reaction function (i.e. direction of change in the optimal cutpoint) if expectations departed from "rational expectations" in a neighborhood of the equilibrium. If that derivative was positive then our prediction was an observed level of contribution greater than the equilibrium level of contribution and if the derivative was negative then we predicted the opposite.

That method of evaluating H has two potential weaknesses. First, the probability bias is measured with respect to the equilibrium prediction, not with respect to the objective frequencies of what players are actually doing. Second, the sign of the derivative evaluated at the equilibrium may be different that the sign of the derivative evaluated elsewhere. That is we are making global comparisons by extrapolating from a local calculation.

To show that these are not serious problems, we next examine whether the expectations overestimated the actual frequencies of contributions. We do this in the following way.

Suppose that we observe an empirical frequency of contributions equal to $\hat{q}$. Ignoring individual differences, this implies a cutpoint rule $c(\hat{q}) = F^{-1}(\hat{q})$ that the players are following. We may then perform the following calculation. First we maintain the assumption that players are using best responses, given their beliefs, so that they are all maximizing under the expectation that others are contributing with probability equal to q^e (possibly not equal to $\hat{q}$). Next we ask "what value of q^e would rationalize the use of the strategy $c(\hat{q})$?" If $q^e > \hat{q}$ then H is confirmed, because it means that $\hat{q}$ can only be justified as deriving from best response behavior if players beliefs, q^e, exceed $\hat{q}$. If $\hat{q} < q^e$ then H is contradicted.

Care is required in comparing $\hat{q}$ and q^e. First, for the 2 of 3 and 2 of 4 experiments, it is possible that $\hat{q}$ is sufficiently high that the implied cutpoint is not a best response to any cutpoint. Thus, any such observations reject the joint hypothesis that subjects are responding optimally to biased priors. Second, in 2 of 3 experiments where a best response can be found, there are two values of q^e that are consistent with the observed cutpoint. If both of these exceed $\hat{q}$, H is unambiguously supported. But the data are ambiguous if only one exceeds $\hat{q}$. Finally, for $w = N = 3$ and $\bar{c} > 1$, we are guaranteed that $q^e > \hat{q}$ unless $\hat{q} = 0$. For experiments with these parameters, H cannot be put to a test.

Table 7 displays $\hat{q}$ and q^e (the latter in parenthesis) for all experiments. The hypothesis H is supported in all 3 of 3 experiments and all but one of the 1 of 3 experiments. In the 2 of 3 experiments, three experiments rejected the joint hypothesis since the value of $\hat{q}$ was too great to be consistent with optimizing behavior. In three other experiments, one q^e was below $\hat{q}$, the other above. In the remaining six 2 of 3 experiments, H was supported. H was not supported in the 2 of 4 experiments. In three of the four such experiments, there is no q^e that rationalizes $\hat{q}$; in the remaining experiment, one of the two solutions is above q^e, the other below.

Summarizing, this alternative examination of the probability bias hypothesis produces somewhat more mixed results than before. While the hypothesis seems to hold for most of the experiments, there are some parameter values for which it clearly fails.

C. Learning

Stronger support for H is found in studying whether the priors q^e move in the direction of q* during the experiment. If players have systematically inaccurate prior beliefs about the likelihood that others will contribute, then as they play the game and observe a sample of contribution rates, one would expect them to adjust their beliefs in light of this new information. This will implicitly lead to a dynamic learning process that will be reflected in changes in the observed q's.

One natural hypothesis is that this adjustment process will produce $\hat{q}$ that are closer to the equilibrium q* late in the session compared to the early rounds of the session. There are 28 cases where q^e can be rationalized in the last 10 rounds. Counting as a success one 2 of 4 experiment and one 2 of 3 experiment that could be rationalized in the last 10 rounds but not in the first 10, we find that 22 of 28 cases move in the right direction.[16]

An alternative hypothesis about learning is that the difference between q^e and $\hat{q}$ is less in the last 10 rounds than in the first 10 rounds In 5 of the 6 remaining experiments, this is the case indicating that even in these experiments there is evidence that subjects are learning. Summarizing the evidence for the aggregate data, the direction of movement of q^e supports the hypothesis that players' priors on q are biased upward initially, but they adjust these priors in the correct direction during the course of the experiment.

Somewhat surprisingly, even at the aggregate level, we find the amount of movement is slight. Table 8 compares frequencies of contribution in the first five rounds with contribution in the last 5 rounds. A "+" sign indicates a higher rate of contribution in the first five rounds. Statistical tests for differences in aggregate behavior over the last 5

compared to the first 5 rounds are generally weak. There are 33 tests, only three of which have p-levels below 0.1.

We are thus left, for now at least, with a successful probability bias hypothesis, together with Bayesian-Nash equilibrium predictions, as the major sources of explanation for the variation in aggregate contribution rates. However, a more definitive statement of the results, especially regarding learning effects would requires a rigorous analysis of the data at the individual level that is beyond the scope of this paper.

Table 1

Description of Experiments

Date	Site	w	N	Range	Cents	$\bar{c}$	Subjs.	Rounds	Reveal	Sequence
4/27/89	CMU	1	3	90	0.7	2.25	12	20	No	3
5/2/89	CMU	1	3	90	0.7	2.25	12	20	No	1
7/26/89	CIT	2	4	204	0.1	2.22	12	17	No	1
7/31/89	CIT	2	4	90	0.3	2.25	12	20	No	3
8/8/89	CIT	2	4	90	0.3	2.25	12	20	Yes	3
4/27/89	CMU	2	3	90	0.7	2.25	12	20	No	1
5/2/89	CMU	2	3	90	0.7	2.25	12	20	No	3
4/27/89	CMU	3	3	90	0.7	2.25	12	20	No	2
5/2/89	CMU	3	3	90	0.7	2.25	12	20	No	2
7/13/88	CIT	1	3	90	0.5	1.5	12	30	No	-
2/15/89	CMU	1	3	90	0.3	1.5	12	20	No	2
2/16/89	CMU	1	3	90	0.3	1.5	12	20	No	2
7/31/89	CIT	1	3	90	0.3	1.5	12	20	No	2
8/8/89	CIT	1	3	90	0.3	1.5	12	18	Yes	2
11/22/87	CIT	2	3	90	1	1.5	9	20	No	-
12/3/87	CIT	2	3	90	1	1.5	9	20	No	-
12/20/87	CIT	2	3	90	1	1.5	9	20	No	-
2/15/89	CMU	2	3	90	0.3	1.5	12	20	No	1
2/16/89	CMU	2	3	90	0.3	1.5	12	20	No	3
7/31/89	CIT	2	3	90	0.3	1.5	12	20	No	1
8/8/89	CIT	2	3	90	0.3	1.5	12	20	Yes	1
7/21/88	CIT	3	3	90	0.5	1.5	12	30	No	-
2/15/89	CMU	3	3	90	0.3	1.5	12	20	No	3
2/16/89	CMU	3	3	90	0.3	1.5	12	20	No	1
8/3/89	CIT	3	3	204	0.1	$\frac{204}{205}$	12	25	No	1
2/21/89	CMU	1	3	90	0.2	3/4	12	20	No	1
3/1/89	CMU	1	3	90	0.2	3/4	12	20	No	3
2/21/89	CMU	2	3	90	0.2	3/4	12	20	No	2
3/1/89	CMU	2	3	90	0.2	3/4	12	20	No	2
2/21/89	CMU	3	3	90	0.2	3/4	12	20	No	3
3/1/89	CMU	3	3	90	0.2	3/4	12	20	No	1
7/26/89	CIT	2	4	204	0.1	2/3	12	25	No	2
7/26/89	CIT	2	3	204	0.1	2/3	12	24	No	3

Notes to Table 1.

Francs - This gives the upper end of the uniform (in integers) distribution of endowments in francs. This number and $\bar{c}$ can be used to calculate the franc value of the public benefit, B = Francs/$\bar{c}$.

Cents - The number of cents paid per franc earned in the experiment.

Subjects - The number of subjects in the experiment.

Rounds - The number of (non-practice) rounds for the given (w, N, $\bar{c}$) parameters. Experiments of 20, 25, or 30 rounds ran for the planned duration. Experiments of 17, 18, and 24 rounds were prematurely terminated by computer crashes.

Reveal - When this parameter is "Yes", all token values were revealed after each round. Subjects could match individual token values and decisions but could not identify the individuals. Otherwise, token values were not revealed.

Sequence - Only one set of subjects was run on a given date. Some sets of subjects played three sets of parameters in sequence. The sequence shows the order in which the parameters were used. In the experiment on 8/3/89, subjects were used, subsequent to their play of a 3 of 3 game, for two other treatments in which groups were fixed rather than rotated. When this entry is blank, subjects were run for only one set of parameters.

Other - The $\bar{c}$ value of 2.22 was for a treatment in the 2.25 condition in which franc values had been rescaled from 90 to 204. Setting B to the integer 91 resulted in $\bar{c}$ = 2.22.

The 18 experiments at CMU form a matched set in which the same randomly drawn endowments were used with all six sets of subjects. Two sets of subjects were run for each of three different values of $\bar{c}$ (2.25, 1.5, and 0.75). Within each $\bar{c}$ value, the order of the w=1, 2, and 3 experiments was permuted so that the first experiment and third experiments were reversed for the two sets of subjects.

The endowments in the CMU 1 of 3 $\bar{c}$ = 1.5 experiments were matched in the 1 of 3 experiment at CIT on 7/31/89. (The three experiments on that date and on 8/8/89 followed the same sequence but differed in endowment assignments and the Reveal treatment.)

Otherwise, new random draws were used in each experiment.

Table 2.

Classification Errors, Bayesian Nash Predictions*

$\bar{c}$	w/N 1/3	2/4	2/3	3/3
$2\frac{1}{4}$	26/240 29/240	49/204 44/240 29/240	46/240 38/340	23/240 31/240
$1\frac{1}{2}$	64/360 33/240 34/240 53/240 35/216		23/180, 30/180 37/180, 31/240 39/240 40/240 68/240	112/360 47/240 60/240
$\frac{204}{205}$				132/300
$\frac{3}{4}$	61/240 61/240		46/240 61/240	126/240 143/240
$\frac{2}{3}$		73/300	67/240	

* The first number for each experiment is the number of classification errors. The second number is the total number of decisions.

Table 3.

Endowments at Least Equal to Benefit: Contributions/Endowments*

$\bar{c}$	w/N = 1/3	2/4	2/3	3/3
$2\frac{1}{4}$	0/141 3/141	6/112 (2/36) 2/139 2/139	6/147 (1/34) 0/147	2/138 0/138
$1\frac{1}{2}$	2/124 (2/24) 0/77 1/77 1/77 2/69		0/61, 0/67 0/61, 1/89 0/89 0/89 12/89 (2/20)	19/127 (2/17) 0/87 4/87

*The first number is the total number of contributions when the endowment was at least equal to the benefit. The second number is the total number of occurrences of endowments greater or equal to the endowment. The number of contributions of this type in the last five rounds of each experiment was zero, except where shown in small font. Reveal experiments shown in boldface.

For the 2x2 comparison spend/not spend vs. endowment ≥ benefit/endowment < benefit, the standard χ^2 test was always significant at $p < 10^{-4}$. The same holds true for the first 10 rounds of each experiment except two experiments where the p-levels were .0003 and .062, respectively and for the last 10 rounds except for two experiments with p-levels .0006 and .0001 (last 8 rounds).

Table 4.

The Frequency of Contribution*

$\bar{c}$	w/N = 1/3	w/N = 2/4	w/N = 2/3	w/N = 3/3
$2\frac{1}{4}$	0.217 (0.192) 0.213 (0.192)	0.309* (0.132) 0.221* (0.104) 0.200* (0.104)	0.192 (0) 0.159 (0)	0.096 (0) 0.129 (0)
$1\frac{1}{2}$	0.311* (0.306) 0.221* (0.271) 0.263* (0.271) 0.333* (0.271) 0.292 (0.269)		0.300* (0.239) 0.333* (0.222) 0.417* (0.244) 0.308* (0.238) 0.308* (0.238) 0.363* (0.238) 0.379* (0.238)	0.311 (0) 0.196 (0) 0.250 (0)
$\frac{204}{205}$				0.440 (0)
$\frac{3}{4}$	0.379 (0.392) 0.404 (0.392)		0.521* (0.596) 0.558 (0.596)	0.525 (0) 0.596 (0)
$\frac{2}{3}$		0.500 (0.523)	0.580* (0.674)	

Note. The entries in parentheses in each cell are the theoretical contribution frequencies for symmetric Bayesian Nash equilibrium under risk neutrality. The frequencies were calculated using the actual token draws in the experiment.The absence of an entry in a cell indicates that no experiments were run for the parameters corresponding to the cell. Actual frequencies that represent deviations from the theory that are not consistent with H are shown in boldface.

* Departure from theoretical frequency statistically significant at 0.05 level on basis of t-test, using Normal approximation to binomial.

Table 5.

The Frequency of Contribution, Endowment Below Benefit Level *

$\bar{c}$	w/N 1/3		2/4		2/3		3/3	
$2\frac{1}{4}$	0.525	(0.465)	0.620*	(0.293)	0.430	(0)	0.206	(0)
	0.485	(0.465)	0.505*	(0.248)	0.409	(0)	0.304	(0)
			0.465*	(0.248)				
$1\frac{1}{2}$	0.466	(0.466)			0.453*	(0.361)	0.399	(0)
	0.325	(0.399)			0.531*	(0.354)	0.366	(0)
	0.380*	(0.399)			0.630*	(0.370)	0.398	(0)
	0.485*	(0.399)			0.483*	(0.378)		
	0.415	(0.395)			0.470*	(0.378)		
					0.576*	(0.378)		
					0.523*	(0.378)		

*See Table 4 for explanatory notes.

Table 6.

Theoretical Contribution Probabilities for Group Optimal Behavior*

$\bar{c}$ \ w/N	1/3	2/4	2/3	3/3
$2\frac{1}{4}$	0.431 (0.25)	0.567 (0.134)	0.625 (0)	0 (0)
$1\frac{1}{2}$	0.5 (0.313)	0.646 (0.293)	0.75 (0.250)	1 (0)
$\frac{204}{205}$	0.567 (0.381)	0.712 (0.424)	0.834 (0.500)	1 (0)
$\frac{3}{4}$	0.610 (0.431)	0.75 (0.500)	0.875 (0.625)	1 (0)
$\frac{2}{3}$	0.627 (0.453)	0.765 (0.529)	0.889 (0.667)	1 (0)

* Note: The lower entries in each cell are the theoretical contribution probabilities for symmetric Bayesian Nash equilibrium under risk neutrality. The upper entries are the contribution probabilities generated by the cutpoint that maximizes expected group payoff.

Table 7

Estimated Prior Probabilities in the Experiments

			All Rounds		Last 10 Rounds	
Date	Site		q^{obs}	q^{e}	q^{obs}	q^{e}
4/27/89	CMU	w=1, N=3, $\bar{c}$=2.25	.217	.302	.242	.263
5/2/89	CMU		.213	.308	.225	.288
7/26/89	CIT	w=2, N=4, $\bar{c}$=2.25	.309	-	.298	-
7/31/89	CIT		.221	-	.217	-
8/8/89	CIT		.200	-	.192	.488
4/27/89	CMU	w=2, N=3, $\bar{c}$=2.25	.192	.315,.685	.183	.291,.719
5/2/89	CMU		.158	.232,.658	.150	.215,.785
4/27/89	CMU	w=3, N=3, $\bar{c}$=2.25	.096	.464	.075	.411
5/2/89	CMU		.129	.538	.108	.493
7/13/88	CIT	w=1, M=3, $\bar{c}$=1.5	.311	.317	.292	.339
2/15/89	CMU		.221	.424	.233	.408
2/16/89	CMU		.263	.394	.250	.387
7/31/89	CIT		.333	.293	.342	.284
8/8/89	CIT		.292	.338	.302	.327
11/22/87	CIT	w=2, N=3, $\bar{c}$=1.5	.300	.342,.638	.311	.371,.629
12/3/87	CIT		.333	.500	.289	.433,.567
12/20/87	CIT		.417	-	.467	-
2/15/89	CMU		.308	.363,.647	.317	.388,.612
2/16/89	CMU		.308	.363,.647	.292	.323,.677
7/31/89	CIT		.363	-	.392	-
8/8/89	CIT		.379	-	.358	-
7/21/88	CIT	w=3, N=3, $\bar{c}$=1.5	.311	.683	.258	.622
2/15/89	CMU		.196	.542	.142	.461
2/16/89	CMU		.250	.613	.175	.512
8/3/89	CIT	w=3, N=3, $\bar{c}$=204/205	.440	.662	.325	.569
2/21/89	CMU	w=1, N=3, $\bar{c}$=3/4	.379	.467	.408	.446
3/1/89	CMU		.404	.449	.408	.446
2/21/89	CMU	w=2, N=3, $\bar{c}$=3/4	.521	.266,.734	.583	.323,.677
3/1/89	CMU		.558	.298,.702	.583	.323..677
2/21/89	CMU	w=3, N=3, $\bar{c}$=3/4	.525	.373,.627	.492	.393,.607
3/1/89	CMU		.596	.332,.668	.575	.343,.657
7/26/89	CIT	w=2, N=4, $\bar{c}$=2/3	.500	.156,.551	.458	.136,.582
7/26/89	CIT	w=2, N=3, $\bar{c}$=2/3	.580	.318,.682	.558	.390,.610

Table 8.

Differences in the Frequency of Contribution:

First Five Trials Frequency - Last Five Trials Frequency

$\bar{c}$ \ w/N	1/3	2/4	2/3	3/3
$2\frac{1}{4}$	$-.03^{0}$ $+.02^{0}$	$+.12^{0}$ $+.10^{0}$ $+.02^{0}$	$+.03^{0}$ $+.02^{0}$	$+.06^{0}$ $+.05^{0}$
$1\frac{1}{2}$	$+.08^{0}$ $-.05^{0}$ $+.05^{0}$ $-.02^{0}$ $-.05^{0}$		$-.02^{0}$, $.00^{0}$ $-.13^{0}$, $-.05^{0}$ $+.08^{0}$ $-.07^{0}$ $.00^{0}$	$+.03^{0}$ $+.07^{0}$ $+.22^{2}$
$\frac{204}{205}$				$+.28^{2}$
$\frac{3}{4}$	$+.03^{0}$ $+.03^{0}$		$-.27^{2}$ $-.05^{0}$	$+.05^{0}$ $.00^{0}$
$\frac{2}{3}$		$+.14^{0}$	$+.10^{0}$	

0 Likelihood-ratio test significant at $p > 10^{-1}$

2 Likelihood-ratio test significant at $10^{-2} > p \geq 10^{-3}$.

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APPENDIX

These are the instructions for the laboratory session run on 7/31/89 at the California Institute of Technology. Subjects were seated in front of computer terminals that were separated by partitions. These instructions were not distributed, but were read aloud to the subjects. In addition, the payoff tables were distributed to the subjects as indicated below.

After reading the instructions for the first experiment, two practice rounds were conducted to familiarize the subjects with the procedures and the computer screens. The practice rounds were very controlled in the sense that subjects were instructed exactly what actions to take in the practice round. They were also shown how to access a "history screen" which summarized the past decisions made and outcomes in the games they had played in previous rounds. After going through these two practice rounds, all subjects were given a quiz to make sure they understood the details of the experiment and how their earnings would be computed. Any misunderstandings were clarified and the experiment commenced.

After the first experiment of the session had concluded, subjects were briefly informed of the new rules for the second experiment, and then the second experiment commenced. Because the keyboard tasks and the screen layouts were similar in all three experiments conducted in each session, practice rounds and quizzes were not always conducted before the second and third experiments. At the end of a session, subjects were paid in private in a separate room. Each subject was then dismissed from the expermiment before the next subject was paid. Similar procedures were followed in the other sessions.

INSTRUCTIONS:

This is an experiment in decision making. You will be paid in cash at the end of the experiment. The amount of money you earn will depend upon the decisions you make and on the decisions other people make. It is important that you do not talk at all or otherwise attempt to communicate with the other subjects except according to the specific rules

of the experiment. If you have a question, feel free to raise your hand. One of us will come over to where you are sitting and answer your question in private. This session you are participating in is broken down into a sequence of three separate expeiments. Each experiment will last 20 rounds. All money is denominated in Francs. At the end of the last experiment, you will be paid $.30 for every 100 Francs you have accumulated during the course of all three experiments.

RULES FOR EXPERIMENT #1

At the beginning of every round of every experiment, you will be randomly assigned to a group with two other subjects. Each round in the experiment you will have a single token to use in one of two ways:

Option #1: Spend the token.

Option #2: Keep the token.

The amount of money you earn in a round depends upon whether you keep or spend your token that round and how many others in your group spend their token. Each round, you will be told how many Francs your token is worth if you do not spend it. This amount, called your token value, will change from round to round and will vary from person to person randomly. To be more specific, in each round, this amount is equally likely to be anywhere from 1 to 90 Francs. There is absolutely no systematic or intentional pattern to your token values or the token values of anyone else. The determination of token values across rounds and across people is entirely random. Therefore, everyone in your group will generally have different token values. Further- more, these token values will change from period to period in a random way. You will be informed privately what your new token value is at the beginning of each round and you are not permitted to tell anyone what this amount is.

Specific instructions:

At the start of each round you are told your token value for that round. Remember that members of the same group will generally have different token values and these values change randomly for every one after

each round. After being told your token value, you must wait at least 10 seconds before making your decision to keep or spend. Your keyboard will be frozen for this period of time. When everyone has made a decision, you are told which members of your group spent their token and what your earnings were for that round. This will continue for 20 rounds. Following each round you are randomly given a new token value, and randomly reassigned to a new group.

Payoffs:

In each round of experiment 1, if at least 2 out of the 3 members in your group decides to spend their token, every member (both spenders and nonspenders) in your group will each earn 60 Francs. In addition, nonspenders in your group also earn their token value. what happens in your group has no effect on the payoffs to members of the other groups and vice versa. Therefore, in each round, you have three possible earnings. These are shown in the following table: [Hand out earnings table.]

Earnings Table for Experiment 1

You Spend	Number of Others Spending	Your Earnings
Yes	0	0 Francs
Yes	1	60 Francs
Yes	2	60 Francs
No	0	Your Token Value
No	1	Your Token Value
No	2	Your Token Value + 60 Francs

Specific instructions for Experiment 2:

This is exactly the same as experiment 1 except that only 1 spender is needed for all members in that group to receive 60.

PAYOFFS

In each round, if at least 1 out of the 3 members in your group decides to spend their token, every member (both spenders and nonspenders) in your group will each earn 60 Francs. In addition, nonspenders in your group also earn their token value. What happens in your group has no effect on the payoffs to members of the other groups and vice versa. Therefore, in each round, you have three possible earnings. These are shown in the following table: [Hand out new table to subjects and collect old table.]

Earnings Table for Experiment 2

You Spend	Number of Others Spending	Your Earnings
Yes	0	60 Francs
Yes	1	60 Francs
Yes	2	60 Francs
No	0	Your Token Value
No	1	Your Token Value + 60 Francs
No	2	Your Token Value + 60 Francs

Specific instructions for Experiment 3:

There are three differences in this experiment. First of all, each group has 4 members instead of 3. Second, 2 out of the 4 members of a group must spend in order for all members of that group to get the extra payment. Third, the extra payment is 40 instead of 60.

PAYOFFS

In each round, if at least 2 out of the 4 members in your group decides to spend their token, every member (both spenders and nonspenders) in your group will each earn 40 Francs. In addition, nonspenders in your group also earn their token value. What happens in your group has no effect on the payoffs to members of the other groups and vice versa. Therefore, in each round, you have three possible earnings. These are shown in the following table: [Hand out new table to subjects, and collect old table.]

Earnings Table for Experiment 3

You Spend	Number of Others Spending	Your Earnings
Yes	0	0 Francs
Yes	1	40 Francs
Yes	2	40 Francs
Yes	3	40 Francs
No	0	Your Token Value
No	1	Your Token Value
No	2	Your Token Value + 40 Francs
No	3	Your Token Value + 40 Francs

Notes

[1]Up to this point, our approach is the same as Rapoport's (1985)expected utility model of the Van de Kragt et al. (1983) experiments. Weimpose game theoretic equilibrium restrictions, which Rapoport does not do.

[2]See, for example, Palfrey and Rosenthal's (1988) altruism model or, inthe context of risk averse behavior in sealed bid auctions, Cox, Robertson,and Smith (1982) and Cox, Smith and Walker (1983).

[3]Variations of the game with different payoff structures can be treated. Similarly, the model can be generalized to allow for a non-zero lower support to F(c) and to F(c) non-uniform. See Palfrey and Rosenthal (1987, 1989). Here we introduce only parameters varied in the experiments reported below.

[4]Since F is continuous and any specific c has zero measure, wewillfrequently simplify matters by being imprecise about knife-edgesituations,in this case c=1.

[5]See Palfrey and Rosenthal (1990) for a theoretical and experimental analysis of the game when preplay communication is permitted.

[6]Moreover, the iterated map defined by $c_{t+1} = G(c_t)$ converges globally to the GESE in all but two cases. In both those cases, $G(\cdot)$ is explosive, but there exist monotone transformations of G that will converge.

[7]Note that if w=1, for any $F(\cdot)$, there exists a unique $q^* > 0$.

[8]Note that for $w < N$, there never exists an equilibrium with $q^* = 1$.

[9]Showing uniqueness is direct with N=3 since $q=F(c^*)$ is linear in c^* while equation (1) is quadratic in q. In the w=2, N=4 game, uniqueness follows from noting that $\frac{dc}{dq} \gtreqless 0$ if $q \lesseqgtr 1/3$ and $\frac{d^2c}{dq^2} \lesseqgtr 0$ if $q \lesseqgtr 2/3$.

[10]This assumption can be relaxed.

[11]In the actual experiment, subjects know prior to play that, post play, they will not only learn their payoffs but also the total number of contributors in the group. It is possible that subjects attach utility to these reports (Is being the sole non-contributor in a unanimity game worse than not contributing when no one else does?), but we choose to ignore this complexity.

[12]The data can be obtained by mailing a request and a 3.5" high density diskette to either author.

[13]These costs were generated in advance by a standard computerized pseudo-random number generator. In the three 1987 experiments, token values were assigned directly in cents. There do not seem to have been any important effects from the manner of presenting or scaling the payoffs.

[14]There do not appear to be important effects associated with either the manner of presenting the payoffs (francs vs. cents) or scaling the payoffs.

[15]Finding pervasive use of dominant strategies is in fact surprising since the same result does not occur in Prisoners' Dilemma experiments. A key difference between our experiments and the Prisoners' Dilemma is that in our experiments "All Contribute" does not Pareto dominate "All not Contribute".

[16]In those cases where there are two solutions for q^e, both solutions move toward q^*.

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